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Comments on statistical and orthodox forecasting

by
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1. About eighteen months ago I wrote a somewhat lengthy paper on the possibilities of statistical forecasting which led, early in 1955, to the experiment described by Mr. Johnson (1955). The results of that experiment seem to be very much what I had anticipated. For instance the standard errors expected on statistical grounds from 24-hour statistical forecasts on London-Gander route with three intermediate observations are set out below:-

Table I — Expected R.M.S. errors of statistical forecasts

	Winter	Spring	Summer	Autumn
500 mb.	13 kt.	12 kt.	9½ kt.	11½ kt.
700 mb.	9 kt.	8½ kt.	6½ kt.	8½ kt.

In my original paper I computed some comparisons. For these I got from London Airport their forecasts of equivalent headwinds for the Shannon-Gander route at 500 mb. and their actuals as estimated from their contour charts for 22nd December, 1952 to 4th April, 1953. I made statistical forecasts also for the same occasions. The result was as follows:-

Table II — Root mean square differences between actuals at 500 mb. measured off contour charts and forecasts prepared in various ways for routes across the Atlantic

	R.M.S. difference	R.M.S. difference after correction for contour errors
Orthodox forecasts (24 hours)	9.9 kt.	8.2 kt.
Statistical forecast (0 hours)	7.7 kt.	5.4 kt.
" " (12 hours)	9.0 kt.	7.1 kt.
" " (24 hours)	10.6 kt.	9.0 kt.

The last column probably gives the true errors, as I found by comparison of London Airport's and Prestwick's contour measurements that the root mean square error of measurements of wind over such a route at 500 mb. was about 5½ kt. (Johnson in Table 2 found the R.M.S. difference between L.A.P. and C.F.O. measurements to be 5.9 which gives a R.M.S. error of one set of measurements to be 4.2 kt.)

In Johnson's Table 3 the R.M.S. errors of his trials are given as follows for 500 mb. for Shannon-Gander route, January to April, 1955.

/Table III

Table III — Johnson's figures for forecast errors

	Standard deviation of forecast errors	R.M.S. error
L.A.P. forecast	11.1 kt.	11.0 kt.
C.F.O. forecast	10.9 kt.	10.9 kt.
Statistical forecast (24 hours)	12.3 kt.	13.6 kt.

It may be noted that, since the actuals were the mean of C.F.O. and L.A.P., the R.M.S. error of the actuals used as yard stick was about 3 kt. and provided there is no correlation between the C.F.O. and L.A.P. errors this would make very little difference to the R.M.S. errors of the forecasts. From these tables one can deduce that the season January to April, 1955 was somewhat more difficult for forecasters than December to April, 1953, (probably because the variability of winds was greater). As Mr. Johnson points out, the effect of bias prejudiced the statistical forecast in 1955 but nevertheless the proportionality of the statistical and orthodox forecasts is broadly the same in the two periods, and so the conclusions I reached in my original paper are not disproved.

2. In the planning of the 1955 experiments I wished to have had forecasts made of equivalent head winds rather than of fixed time route winds, for then by comparison with aircraft observation we could have gained knowledge of whether the contour charts drawn at C.F.O. and L.A.P. were both in error in the same sense. However, the estimation of the equivalent head winds presented such difficulties that that scheme was deemed impossible.

However, I obtained from Prestwick the logs of a considerable number of aircraft flying from Gander to Prestwick together with estimates of the actual winds which would have been expected from the contours allowing for the differences in time. The aircraft observations were selected so that two or more were crossing at the same time. Thus (after a statistical correction for small differences in time and position) there were comparisons available (a) between winds found by two aircraft and (b) between winds found from contours and by aircraft. Two periods were examined in this way; in one there were 64 pairs of aircraft, in the other 72 pairs. If we assume that the errors in the aircraft measurements are uncorrelated with each other and also with the contour measurements we can obtain estimates of the probable errors in the aircraft observations and in the geostrophic winds. Further, on 34 occasions I got two estimates made of the geostrophic winds off independent maps by independent estimators. The result was as follows:-

Table IV — R.M.S. of vector errors of winds over a route across the Atlantic at 500 mb. determined in various ways

	Period 1	Period 2	Total
R.M.S. error in aircraft winds	4.8 kt.	8.8 kt.	7.2 kt.
R.M.S. error in geostrophic winds	6.4 kt.	8.8 kt.	7.9 kt.
Apparent R.M.S. error in geostrophic wind derived by comparison of two independent estimates	-	-	7.0 kt.
Number of aircraft pairs	64	72	136

The conclusion which can be drawn from this table is that the winds measured by aircraft over such a route have about the same error as that made by the meteorologist in estimating from his charts and secondly by the comparison of 7.9 kt. and 7.0 kt., that about four fifths of the meteorologist's error is due to incorrect drawing of the contour lines and inaccurate measurements but that one fifth (in this case equivalent to a R.M.S. error of $3\frac{1}{2}$ kt.) is due to consistent errors in drawing, i.e. to faulty pressure readings or to lack of observations or other such biases which lead to the meteorologist's maps being incorrect, however skilfully drawn.

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It is of interest that such consistent errors do exist but in the present experiment one can easily see that they do not materially modify the result.

3. British European Airways Corporation requested in 1951 that 33-hourly forecasts of route winds should be given for a number of routes over the Continent. These forecasts were made by orthodox methods and by the use of a regression equation, the latter being done by an assistant without recourse to a highly trained scientist.

Initially a trial was made and showed that the forecasts from the regression equation were less in error than those by orthodox methods and the regression equation was adopted as a routine. Later, however, after more effort had been put into the preparation of the orthodox forecasts a further comparison was made with the following result.

Table V — Comparison of forecasts for 33 hours ahead made by orthodox and statistical methods

Route	Root mean square errors	
	Orthodox forecasts	Statistical forecasts
London to Bordeaux	13.1 kt.	15.7 kt.
London to Nice	12.5 kt.	12.8 kt.
London to Copenhagen	17.1 kt.	18.7 kt.

The experiment was abandoned because B.E.A.C. operators ran into difficulties, not because the forecasts were found to be of no value.

4. From these examples it is clear that the improvement of orthodox forecasts over statistical is not very great. From Table II and other examples quoted in my original paper it seems that the statistical forecasts could improve on the accuracy of the orthodox forecast if the data on which it were based were 12 hours later than those of the orthodox forecast, and would be but little worse than the orthodox forecast if the data were 6 hours earlier. The time taken to produce a statistical forecast is of the order of 10 minutes after the last observation has been received; the time taken to produce an orthodox forecast is much greater. The orthodox forecasts in this trial were completed before 0300 G.M.T. and referred to an epoch 12 hours later; the statistical forecasts were based on 2100 G.M.T. (or 1500 G.M.T.) observations and an allowance of 6 hours was given for transmissions. It should further be noticed that the orthodox forecasts issued between 0300 G.M.T. and 0900 G.M.T. would be based in the main on the same information as that issued at 0300 G.M.T. whereas the statistical forecast could be brought up to date as each fresh piece of information was received. An improvement of the speed of transmission of data from selected points on the routes (e.g. from weather ships for the Atlantic route) would improve the statistical more than the orthodox forecasts.

5. Apart from the greater speed of the preparation of the statistical forecasts, the advantage of the method is likely to be found (a) in forecasting winds at high levels where the orthodox prontours become increasingly inaccurate and take longer to prepare, (b) in forecasting winds for assessing pay loads; these forecasts (b) are required 24 to 36 hours ahead, they are not operational, and could be made as a routine by assistants, thus saving highly paid staff such as had to be used in the B.E.A. experiment to compete with the accuracy of the forecast by regression equations.

Reference

Johnson, D.H. 1955 Meteorological Research Papers, Air Ministry, London, No. 953.